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THE INSTITUTE OF PHYSICS

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# Light waves, radio waves and photons

RICHARD M. SILLITTO
B.Sc., F.Inst.P.
*University of Edinburgh*

[A lecture given to the Scottish Branch in Edinburgh on November 9, 1959, and in Glasgow on November 10]

In 1900 Planck solved the problem of constructing a formula to represent the distribution in energy of the radiation in a uniform temperature enclosure. The formula he obtained seemed to imply that the exchange of energy between the radiation field and the walls of the enclosure must take place in discrete amounts or *quanta*, the size of the quantum at the (angular) frequency $\omega$ being $\hbar\omega$, where $\hbar \approx 10^{-27}$ erg sec. Einstein's subsequent interpretation of the photoelectric effect (1905) also rested on the assumption that light of (angular) frequency $\omega$ is absorbed by a photosensitive surface in amounts $\hbar\omega$. Whether light energy actually travels in the form of bundles or quanta was open to question, but there was no reason to suppose that it does not.

In this lecture I want to talk about some of the experiments which were expected—or might have been expected—to demonstrate quantum structure in the electromagnetic field.

Two such experiments were reported in the *Proceedings of the Cambridge Philosophical Society* in 1909, one by G. I. Taylor, the other by N. R. Campbell, and some very recent experiments of considerable refinement have close affinities in principle to one or other of those experiments of 50 years ago.

Taylor's experiment[1] was intended to test a suggestion made in 1907 by J. J. Thomson[2] : that the phenomena of ionization by X-rays—and also, we may add, of photoelectricity—might be understood if the energy of electromagnetic waves was supposed to be concentrated in narrow tubes, rather than spread uniformly over a wavefront. The intensity of the radiation would be represented by the average density of the tubes, while even in a radiation field of low intensity the concentration of energy in one such tube could be high enough for an atom to pick up the energy required for ionization very quickly. Now according to the wave theory of light the diffraction pattern formed on a screen by light passing an opaque object can be calculated by summing the effect due to those zones of the wavefront which pass very near the obstacle. In making this calculation it is assumed that the amplitude of the wave is uniform over these zones. Thomson had the idea that if the light intensity was reduced sufficiently the number of tubes passing through each zone would be so small that there would be marked fluctuations of intensity from zone to zone and consequently the appearance of the diffraction pattern would be altered—the *visibility* of the pattern would presumably be reduced. G. I. Taylor therefore photographed the diffraction pattern of a needle for a range of light intensities : at the lowest intensity he used, an exposure of three months was needed. "In no case," he reported, "was there any diminution in the sharpness of the pattern." During his longest exposure the mean energy density of radiation within the apparatus was about $1{\cdot}6 \times 10^{-16}$ ergs/c.c. (or about $2 \times 10^{-4}$ of a Planck quantum). "According to Sir J. J. Thomson," Taylor's paper concluded, "this value sets an upper limit to the amount of energy contained in one of the indivisible units [i.e. tubes of radiation] mentioned above."

This experiment is of great importance for the interpretation of quantum theory, and after the advent of quantum mechanics in 1925 two experiments of a more critical design were performed by Dempster and Batho[3] (1927). The first of these showed that the diffraction images formed by an echelon grating retained their sharpness when the light intensity was reduced so that on average less than one quantum at a time was passing through the diffraction apparatus. Since the entrance aperture of each plate of the echelon grating was about 32 mm$^2$ this set a lower limit to the area of the wavefront which a quantum appears to occupy ; in passing we may note that this minimum area is enormously greater than the area of the atom to which the quantum can give up its energy in the photoelectric process. Dempster and Batho's second experiment showed that a normal interference pattern was formed by the partial reflection of light from two slightly inclined glass plates with a light source so weakly excited that only one atom at a time was radiating in any direction.

Most recently this question of the interference of light at very low intensities has been reinvestigated in Budapest by Jánossy and Náray[4] (1957). They built, in a tunnel 30 metres underground, a Michelson inter-

ferometer whose arms were almost 14 metres long, enormously greater than any characteristic length such as a *coherence length* that might be assigned to a quantum. If interference effects appear with such an apparatus for light intensities so low that only one photon at a time can be passing through the apparatus, the formation of the interference pattern can be interpreted neither in terms of interference between different photons, nor in terms of a pseudo-classical model of photons as wave-packets of limited extent. The interference fringes were recorded by scanning a photomultiplier tube across the interference pattern, and were found to be independent of light intensity. The work of Jánossy and Náray is an outstanding piece of experimentation and verifies conclusively the result first obtained by G. I. Taylor.

The results of this sequence of experiments might be summarized by saying that optical interference is a " single quantum process," in the sense that photons do not interfere with one another to produce interference patterns. It is interesting that an analogous conclusion can be stated for electron interference and diffraction. Our President, Sir George Thomson, pointed out in his book *The wave mechanics of free electrons*[5], and it was verified in 1949 by Fabrikant *et al*[6], that sharp electron diffraction patterns are obtained with electron beams so weak that only one electron at a time passes through the apparatus.

Now let us return again to the year 1909, and consider the experiment designed by N. R. Campbell to investigate " discontinuities in light emission." The two papers[7] in which he reported this experiment provide fascinating reading, and not only because the first

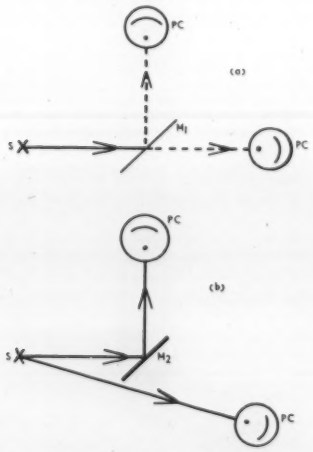


**Fig. 1.** *S—light source ; PC—photoelectric cell ; $M_1$—half-silvered mirror ; $M_2$—mirror.*

begins : " This is an account of an experiment which failed," and the second reported that " the experiment has continued unsuccessful " ! After explaining that such a report has value, if only as a warning to others, Campbell analyzed with beautiful clarity the implications for what we would call " coherence " of Thomson's idea of *tubes of radiation*, and of Stark's *bundle-of-energy* interpretation[8] of the quantum hypothesis. He then suggested that the coherence of two beams of light might be investigated by a study of the correlation between the fluctuations in the outputs of two photoelectric cells placed one in each beam—a proposal which, in those very early days of the study of discontinuous phenomena, could surely have originated with no one but N. R. Campbell. Referring to Figure 1, it seemed that if Thomson's theory were correct the beams falling on the photocells in the arrangement of Figure 1a might be " dependent ", while in the arrangement of Figure 1b they might be " independent " ; on the *bundle-of-energy* theory—which resembled the rather crude photon model which still we sometimes use too naïvely—Campbell thought the correlations in the two arrangements would be the same. With the two cells connected as in Figure 2, and the potentiometer balanced, any light fluctuation

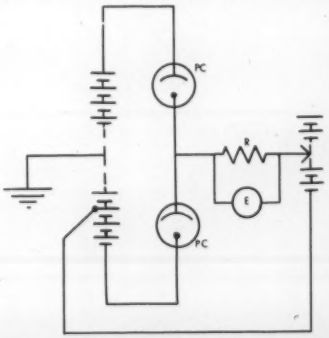


**Fig. 2.** *PC—photoelectric cell ; R—high resistance ; E—electrometer.*

which affected both cells simultaneously would not affect the balance, and no current should flow in the high resistance $R$, but uncorrelated fluctuations in the two beams would cause a fluctuating voltage to develop across $R$, and so would affect the electrometer. Campbell's experiment failed—as he shows in papers which analyse carefully and frankly its every aspect, theoretical and technical—because of the erratic behaviour of his light sources, and all his efforts could not overcome this technical difficulty. But what a brilliantly conceived experiment, and what a remarkable pair of papers !

Within the last ten years, the effects which Campbell hoped to investigate have been studied intensively, and the interpretation of the experimental results was for a

year or two the subject of controversy. If the photocells of Fig. 1a are replaced by photomultiplier tubes, evidence for the possible correlation between the outputs of the photomultipliers may be sought in two ways. In the first, suggested by Bay and realized by Ádám, Jánossy and Varga[9], the pulses produced at the anodes by the emission of individual photoelectrons from the cathodes are fed to counters, and also to a coincidence detector. If the resolving time of the coincidence detector is known it is easy to calculate, from the counting rates in the two photomultipliers, the coincidence rate which should be observed if events in the two counters are random and uncorrelated. Any excess of the observed coincidence rate over the calculated coincidence rate will then show that there is a positive correlation between the outputs of the two multipliers. In the second detection technique, which uses higher light intensities, the alternating components of the photomultiplier output currents are fed into a correlation motor—in effect a sensitive integrating a.c. wattmeter whose two windings are fed from the outputs of the two photomultipliers—and any correlation between the photomultiplier outputs will cause the fluctuations in position of the motor shaft to be about a mean which moves progressively in one direction. If the correlation is small its effect may not be seen : in the first arrangement because it may be masked by the random fluctuations in the counting rates, in the second arrangement because the random fluctuations of the position of the motor shaft may conceal the slight advance of its mean position. In fact the first experiment by Jánossy and his collaborators gave a null result for this reason, and it was Brown and Twiss[10], using the second technique, who demonstrated that there is a positive correlation between the photomultiplier outputs, provided the beams falling on the cathodes are coherent, i.e. capable of producing an interference pattern if superposed ; further the magnitude of the correlation depends on the degree of coherence of the light beams in the same way as does the visibility of the interference pattern they would produce if recombined. Subsequently the coincidence-detector technique was improved by a number of workers[11], and gave results consistent with those first obtained by Brown and Twiss.

It is one of the interesting features of this result that it cannot be understood in terms of the crude—too crude !—model of a beam of light as a stream of discrete, indivisible, corpuscular photons, just as the result of " Taylor's experiment " cannot be understood in terms of this model ; but like Taylor's observation of interference at very low light intensities the observation of Brown and Twiss can be explained in a quite classical way. Indeed we can say that it is in the nature of the quantum theory of radiation that the classical explanation is the correct one. When a beam of electromagnetic waves is split by a half-silvered mirror any intensity fluctuations in the beam, arising from the light source, propagate in both components of the split beam. They should therefore be expected to give rise to correlated fluctuations in the photocurrents generated when the two beams fall on photosensitive cathodes. The quantal nature of the photoelectric interaction shows in the fact that the photocurrent does not vary in exact proportion to the instantaneous light intensity, but depends on the intensity in a way which is only statistically determinate. What is proportional to the intensity is not the *rate* of emission of photoelectrons, but the *probability* of emission of photoelectrons. Consequently the photocurrent tends to follow the fluctuations in the intensity of the electromagnetic field, but there are also random (Poissonian) fluctuations in the photocurrent, which we call " shot noise." The correlated fluctuations are difficult to observe, because even under the best experimental conditions they tend to be small compared with the uncorrelated shot noise fluctuations.

The classical description of the effect of the fluctuations in the field intensity uses methods familiar in the theory of random signals[12]. If the normalized spectral density of the radiation is $g(\nu)$, and if $\rho(\tau)$ is the Fourier Transform of $g(\nu)$, the temporal fluctuations of the field intensity $P$ can be characterized by the correlation function

$$\overline{P(t)\,P(t+\tau)} = \overline{P}^2(1+|\rho|^2).$$

If a plane-polarized field of intensity $P$ ejects $\alpha P\delta t$ photoelectrons from a cathode in time $\delta t$—N.B. : classical causality is implied here !—the mean number of photoelectrons ejected in time intervals of duration $T$ is

$$\bar{n}_T = \alpha\,\overline{P}\,T, \text{ so that } \bar{n}_T{}^2 = \alpha^2\,\overline{P}^2\,T^2.$$

On the other hand

$$\overline{n_T{}^2} = \alpha^2 \int_0^T \int_0^T P(t)\,P(t')\,dt\,dt'$$

$$= \alpha^2 \iint P(t)\,P(t+\tau)\,dt\,d\tau$$

(within the corresponding limits)

$$= \alpha^2\,\overline{P}^2\,T^2 + \alpha^2\,\overline{P}^2\,T \int_0^T |\rho|^2 \left(1-\frac{\tau}{T}\right) d\tau.$$

Now if $\int_0^T |\rho|^2\,d\tau$ is written as $\tau_0$, and if $\tau_0 \ll T$ so that the second term under the integral may be neglected, this becomes

$$\overline{n_T{}^2} - \bar{n}_T{}^2 = \alpha^2\,\overline{P}^2 T^2 \times \frac{\tau_0}{T} = \bar{n}_T{}^2\,\tau_0/T,$$

which is the mean square fluctuation in the electron emission due to the fluctuations in the field, which arise in practice from the random nature of the light emissions. If then we add to this " classical " fluctuation a " quantal " term to account for the random character of the photoelectric effect, which is a Poissonian process with variance $\bar{n}_T$, we get finally

$$\overline{\sigma_T{}^2} = \bar{n}_T + \bar{n}_T{}^2\,\tau_0/T = \bar{n}_T\,(1+\bar{n}_T\,\tau_0/T). \qquad (1)$$

It is worth remarking on the similarity of this result to the quantum-statistical expression[13] for the mean square fluctuation in a radiation field of frequency $\nu$ :

$$\frac{\overline{\Delta E^2}}{h^2\nu^2} = N_p + N_p{}^2/N_\nu.$$

Here $N_p = \overline{E}/h\nu$ is the number of quanta present, so $\overline{\Delta E^2}/h^2\nu^2$ can be written as $\overline{\Delta N_p{}^2}$, and we have

$$\overline{\Delta N_p{}^2} = N_p + N_p{}^2/N_\nu = N_p(1+N_p/N_\nu). \qquad (2)$$

$\mathcal{N}_\nu$ is the number of wave-modes of frequency $\nu$ which are available in the space occupied by the field, so the ratio $N_\nu/\mathcal{N}_\nu$ indicates the degree of degeneracy of the radiation. On the other hand the ratio $\tau_0/T$ of equation (1) involves the " coherence time " $\tau_0$, so equations (1) and (2) together show a connection between optical coherence and the quantal property of degeneracy.

Equation (1) was first obtained by Purcell[14] ; for unpolarized light it becomes

$$\overline{\Delta n^2} = \bar{n}(1 + \tfrac{1}{2}\,\bar{n}\,\tau_0/T) \qquad (1)'$$

since the fluctuations in orthogonal polarizations are independent. From this Purcell showed that in coincidence-counting experiments the additional wave-interaction term in (1)'—additional, that is, to the Poissonian fluctuation represented by the first term—would increase the coincidence rate by a factor $(1+\tau_0/2T)$, which would be small since in practice the coherence time for intense light sources is very small compared with the resolving time $T$ of the detector circuits. In the correlation experiment of Brown and Twiss the wave-interaction fluctuation gives a cross-correlation $(\Delta n)_1\,(\Delta n)_2 = \frac{1}{2}\bar{n}^2\,\tau_0/T$ between the outputs of the two multipliers ; again this term is small.

It must be emphasized that these effects arise from the classical wave features of the radiation field, and they are derived and discussed by Jánossy in a paper entitled " On the classical fluctuations of a beam of light "[15] ; the classical calculation assumes that the Fourier components of the electromagnetic field can be freely superposed, and this supposition is consistent with quantum mechanics since radiation quanta obey Bose-Einstein statistics.

Another class of experiments which illustrate optical correlation is concerned with the production of beats between light waves of slightly differing frequencies. In 1955 Forrester, Gudmundsen and Johnson reported an experiment[16] showing beats between the components into which the *Hg* green line ($\lambda = 5461$Å) is split in a magnetic field. The beat frequency was in the microwave region, and was detected by feeding the output from a photocell into a microwave receiver. The coherence of the radiation was very low : the signal:noise ratio was only $10^{-4}$, and the observation of the effect was a remarkable achievement. In 1959 a rather different and very elegant experiment by Dodd, Fox, Series and Taylor was reported[17], which showed optical beats with a high degree of coherence ; in this experiment coherence is obtained by controlling with an intense r.f. field the phases of the excited atomic states whose radiations subsequently beat together.

The results of all these experiments lead to the conclusion that interference and correlation experiments can yield no information about the quantum character of the electromagnetic field. Indeed, there are good quantum mechanical reasons for this, pointed out by Heisenberg thirty years ago in his Princeton lectures. In rather simpler terms, the argument can be stated thus:

> Between the conjugate energy ($E$) and time ($t$) variables there is an uncertainty relation $\Delta E.\Delta t \geqslant \hbar$, stating a limitation, inherent according to quantum mechanics, on the degree of precision with which we can specify the state of a system characterized by these variables. If the system is a radiation field of (angular) frequency $\omega$, its energy $E$ is $n\hbar\omega$, $n$ being the number of quanta present, while the phase ($\varphi$) of the field is related to time through $t = \varphi/\omega$. Then the above uncertainty relation can be written
>
> $$\Delta n.\Delta\varphi \geqslant 1.$$
>
> If we now consider two light beams of the same frequency a relation of this type defines the limitations on our knowledge of the state of each beam :
>
> $$\Delta n_1.\Delta\varphi_1 = k \text{ (say)}$$
>
> and $\Delta n_2.\Delta\varphi_2 = k.$
>
> If the two beams are superposed, the *total* number of quanta present is $N = n_1 + n_2$, and the *relative* phase is $\Phi = \varphi_1 - \varphi_2$ ; then
>
> $$\Delta N.\Delta\Phi = \Delta(n_1+n_2)\,.\,\Delta(\varphi_1 - \varphi_2)$$
> $$= \Delta n_1.\Delta\varphi_1 - \Delta n_2.\Delta\varphi_2 - \Delta n_1.\Delta\varphi_2 + \Delta n_2.\Delta\varphi_1.$$
>
> The last two terms vanish, for anything else would imply that the measurement of phase or energy in one beam of light affects the state of a different beam, which would be absurd. Hence
>
> $$\Delta N\,.\,\Delta\Phi = \Delta n_1\,.\,\Delta\varphi_1 - \Delta n_2\,.\,\Delta\varphi_2$$
> $$= k - k$$
> $$= 0.$$
>
> Thus the *total* energy and *relative* phase of two interfering light beams may be measured with the precision allowed by classical wave physics, and any experiment which measures these variables, such as an interference or correlation experiment, will give the results predicted by classical theory.

Heisenberg concluded[18] : " the classical wave theory is sufficient for the discussion of all questions of coherence and interference."

In radio waves quantal effects should be even more difficult to detect, for the quantum energy is very small—about $10^{-5}$ eV at centimetre wavelengths, compared with 2 eV in the visible spectrum—and the density of quanta in detectable r.f. fields is rather high. The random motions of the electrons in a receiving aerial generate a noise signal, whose power in the bandwidth $\delta\omega$ is $kT\delta\omega/4\pi$ when the aerial temperature is $T°K$. At room temperature this amounts to about $10^{-21} \times \delta\omega$ watts. If a signal received by the aerial is to exceed the noise-rate of energy transfer between aerial and field the number of quanta of angular frequency $\omega$ transferred to the aerial by the signal in each second must exceed

$$\frac{kT\delta\omega}{4\pi\hbar\omega} \approx 10^{13} \times \frac{2}{Q}$$

where $Q$, the quality factor of the aerial, might be about 100. The response time of the aerial is $2\pi Q/\omega$, so the number of signal quanta absorbed in the response time must be greater than $10^{14}/\omega$. Even at microwave frequencies, where $\omega \approx 10^{11}$, this number of quanta is quite large. Now if the signal is to have a definable phase the energy $E$ in the signal must be large enough to

enable time measurements to be made with a precision very much better than one period, i.e. $\Delta t = 2\pi/n\omega$ where $n$ is a large number. The uncertainty relation for energy and time then shows that

$$\Delta E \geqslant \frac{n\hbar\omega}{2\pi}$$

i.e. the field must contain a large number of quanta. Since the field we are concerned with here is the field interacting with the detector, this means that a large number of quanta must fall on the aerial during its response time. As we have seen, a radio signal giving a signal:noise ratio greater than one satisfies this requirement, so a single radio beam has in practice a measurable phase, and can be described macroscopically, i.e. in the language of classical physics.

Nevertheless experiments have been made at radio frequencies in the hope of exhibiting quantal effects. Some of the experimenters have argued thus :

" Elementary quantum theory shows that bound material systems—atoms and molecules, for instance—have discrete energy levels. The emission of quanta with sharp energies from such systems does not then establish unequivocally that quantum structure is an essential property of radiation ; it may merely show that in certain circumstances information about the quantization of the material system can be propagated electromagnetically. The only measurements in the optical region which are not vulnerable to this attack are measurements of photoemission, in which although the ejected photoelectrons are free electrons their energy is nevertheless determined by the frequency of the incident light ; and measurements of the black-body radiation spectrum, for here the shape of the spectrum is independent of the materials in the walls of the cavity and must therefore reflect an intrinsic property of radiation. Further direct evidence of the quantum structure of electromagnetic radiation should therefore be sought in the interaction between electromagnetic fields and free electrons." (The Compton effect, of course, furnishes such evidence.)

One such experiment was performed by Faragó and Marx (1954)[19], who fired a beam of electrons across an excited microwave cavity and examined the spreading of the emergent beam. If the electron beam is fired through the cavity in the $z$-direction, and the cavity is excited in a $TE_{0mn}$ mode in which the electric field is in the $x$-direction and is independent of $x$, the $x$-component of the force on an electron is $-e(\mathbf{E}+\mathbf{V}\wedge\mathbf{B})$, which is easily shown to be $-e\dfrac{d\mathbf{A}}{dt}$, $\mathbf{A}$ being the vector potential. The net $x$-component of momentum imparted to an electron during the transit of the cavity is then

$$p_x = \int_{transit} F_x dt = -e\int_{transit} d\mathbf{A} = 0 \ ;$$

the integral is zero because the vector potential vanishes at the boundaries of the cavity. Hence, although the electric and magnetic forces acting on the electrons might broaden the beam, they would in the end produce no change in the *directions* of the electron trajectories—at any rate according to this classical argument.

But suppose one argues[20] that the interaction between an electron and the field takes place through the absorption and emission of quanta of energy $\hbar\omega$ : these are quanta of radiation polarized in the $x$-direction, and must therefore modify the $x$-component of the momentum of the electron. If the elementary acts of emission and absorption are independent, the number imparting " upward " momentum might not exactly balance the number imparting " downward " momentum during the transit of the electron, and for this reason the emergent beam would show an angular divergence. In fact this is not observed. The elementary emission and absorption events are not independent, being induced transitions controlled by the macroscopic field in the cavity, and a rigorous quantum mechanical discussion by Fogarassy[21] has shown that the null result of the experiment is to be expected.

A quite different type of radio frequency experiment was proposed by L. B. Slepyan (1957)[22]. His suggestion was that observations should be made of the strength of the signal received from a transmitter by a very sharply tuned receiving aerial, and particularly of the way in which the received signal strength varies as the transmitted power is reduced to a very low level. Picturing the incoming signal as a stream of photons Slepyan argued that at low intensities the power spectrum of this train of impulses would broaden, because of their random distribution in time, and that a decreasing proportion of the received power would lie within the pass-band of the aerial as the photons became more widely spaced. He calculated that the effect might appear in a 3 cm wavelength receiver whose pass-band was less than 24 cycles/sec.—a bandwidth which is not unattainably low[16] [23].

In fact, though, Slepyan's picture of the working of a radio aerial is not in accordance with quantum mechanics, which gives a much more classical account of what happens. It was pointed out by Fürth in 1951[24] that the reception of signals by a radio aerial is an induced absorption under the action of a macroscopic field round the aerial. We can show from quantum mechanical arguments[24] [25] that the net rates of emission and absorption of energy by an assembly of electrons in, say, the walls of an aerial cavity are respectively

$$\left.\begin{aligned} \overline{S_e} &= \frac{4\mu_0\, e^2\, W_{mn}{}^4}{3c} \times \left\{ (N_{mn}+1) \sum_{m,n} W(E_m)\ (r_{mn})^2 \right. \\ \overline{S_a} &= \left. \left\{ N_{mn} \sum_{m,n} W(E_n)\ (r_{mn})^2 \right. \right. \end{aligned}\right\} \quad (3)$$

where $W(E)$ represents the distribution-in-energy of the conduction electrons in the aerial, $er_{mn}$ is the dipole moment of a conduction electron for a process in which a quantum of energy $\hbar\omega_{mn}$ is emitted or absorbed, and the summation is over all the possible transitions between pairs of states whose energies differ by $\hbar\omega_{mn}$ ; $N_{mn}$ is the number of quanta of the appropriate frequency in the cavity. Suppose the size of the cavity is such that its fundamental frequency is $\omega$. When a signal of the frequency $\omega$ is incident on the cavity the number of quanta of that frequency in the cavity grows with the time-constant of the aerial circuit, which is $2\pi\, Q/\omega$.

A dynamic equilibrium, between the number of quanta entering the cavity both from the incident radiation field and from the cavity walls, and the number removed from the cavity by absorption, is attained when the number of quanta accumulated in the cavity is very nearly equal to the number entering it in the response time $2\pi Q/\omega$. As we have already seen this number is large if the signal : noise ratio is greater than one, and under these conditions the aerial and the field with which it interacts behave in a classical way ; in fact it is generally true that classical behaviour is observed whenever the probability of induced transitions is enormously greater than the probability of spontaneous transitions (i.e. whenever $N_{ms} \gg 1$ in equation (3) ) and this will always be so in radio frequency experiments at normal temperatures.

The main conclusion from all these experiments is that the domain of validity of classical electromagnetic theory is very extensive, and that we should be very hesitant about accepting arguments which rely on the " corpuscular photon " model—at any rate beyond the point where the photons are required to do more than obey the laws of conservation of energy and momentum. In fact, I think that if we abolished the word " photon " from our vocabulary for ten years, we should find that we could get on perfectly well without it.

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# The aesthetic design of scientific instruments

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ONE of the saddest features of present day educational establishments is the exclusiveness of the separate fields of knowledge which exist inside them. This exclusiveness is found not only between the fields or subjects, but also between the section's of the subjects themselves. However, it is nowhere more apparent, and unfortunately well established, than between the two broad fields of activity—art and science. As a result of this, artists and scientists tend to be as remote from each other as the average Greek and Japanese—and for the same reasons too, for differing languages and traditions are barriers to mutual communication. The full disadvantage of this chronic educational malaise is nowhere more apparent than in the field of design. For instance, here, the work of architects may present ideas for buildings which are aesthetically acceptable in appearance, but also—unwittingly—internally inconvenient, uneconomic to produce, and dependent on the structural engineer's intervention and expertise for their materialization—and sometimes correction; while the work of structural engineers may be the channel through which economic and structurally efficient, but at the same time appallingly ugly constructions spread far and wide all over the country—an all pervading dilution of the concentrate of ugliness at present to be found in Victorian industrial towns. This same disadvantage may also be seen in the design of some instrument dials and meter faces, where the advantages of precision and mechanical accuracy are sometimes cancelled by the disadvantage that illegibility makes them inaccurate in use. Sometimes this disadvantage leads directly to tragedy—shown by the cases where inquests on the causes of aircraft or railway disasters bring to light instances of misread dials or unseen signals.

This negation of mechanical accuracy is generally due to the instrument designer's professional technical training. As a result, he often designs meter and other dial faces on a drawing board. He seldom has to use them himself under a wide variety of working conditions.

It is not surprising, therefore, to find meters where the spearlike business end of moving pointer successively and completely obscures both numerals and scales as it passes over them; where an "Islamic" inspired

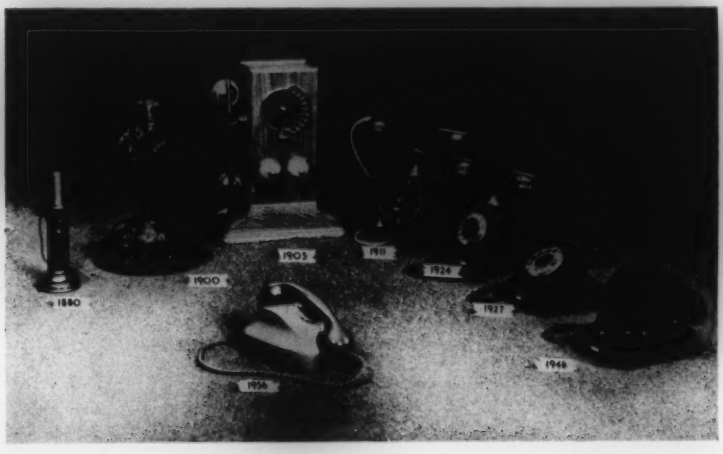



Fig. 1. *Stages in the development of the telephone instrument from 1880 to 1950. Figurative representation, typified by the Graeco-Roman pillar in the earliest model, is gradually replaced by the unified abstract form of the final stage (Siemens & Halske, W. Germany).*

Fig. 2. *By 1956 aesthetic, ergonomic and technical developments carried the development of the telephone instrument still further. In the "Ericfon", except for the bellset and capacitor, all mechanisms including the dial are in the "standset," and all of these except the receiver, are concentrated in the wide lower part (Swedish Ericsson Co., Ltd.)*

crescent moon shaped counterweight draws more of an operator's attention than does the pointer; or where the numerals and scales are difficult to read from the distance or at the speed at which the instrument is used. When this kind of situation arises, it is because the mechanical or electrical engineer turned instrument designer lacks the knowledge of the applied psychologist and the artist typographer. He lacks knowledge—well known even in Caxton's time—on the length of printed line comfortable for scanning; the visually convenient thickness of letters and numerals comparable to their height; the space ratios between letters in words, and between words in sentences; and the general paramount importance of legibility in the transfer of information to the brain through graphic symbols and the human eye.

This separation of knowledge into exclusive departments has not always existed. Galileo—who with admirable intellectual toughness affirmed the theories of Copernicus in the face of a powerful and omniscient Inquisition—was not only an admirable scientist and gifted musician, but at the outset of his career was undecided whether to become a painter or a scientist. During the Rennaissance, many distinguished and gifted men, such as, for example, Leonardo da Vinci and Brunelleschi, were very able in what we would now regard as arts and sciences. In our own country, Sir Christopher Wren, the distinguished designer of St. Paul's Cathedral, London, was an architect, a mathematician and an astronomer. Imagine what would be the present fate of Wren if he had been a pupil in one of our mid-twentieth century secondary grammar schools! In these institutions—hardly designed to cater for genius or first-rate ability—a pupil such as Wren would be listed among a group of difficult boys—difficult because no mental lopsidedness would aid easy classification. When the time came for the educational "parting of the ways," for the decision to be made on the number and names of the subjects of his General Certificate of Education course, the effects of social propaganda combined with a grave plea for his soul by the science and mathematics staff would make his early specialization in the sciences almost inevitable. There would be a very strong possibility that he would "drop" art. Eventually he would combine a possibly distinguished scientific career with a personal tendency to become progressively aesthetically moribund. In the process we would lose a kind of architect of which we are greatly in need of to-day when architecture is becoming industrial design. And what of him? He would not realize the full development of his latent possibilities. Something which education might reasonably be expected to enable him to achieve.

This then is the background against which we have to consider the aesthetic design of scientific instruments. If they are designed by technologists, they will tend to lack the humanism which the applied psychologist and the artist designer would insist upon. In turn, the applied psychologist, who is not trained to be creative, and the artist designer both lack the technological

Fig. 3. *Microscope by George Adams (1704-1773) one of the greatest English Scientific instrument makers of the Georgian period. His designs are a fusion of usefulness, fine finish and elegant figurative treatment.*

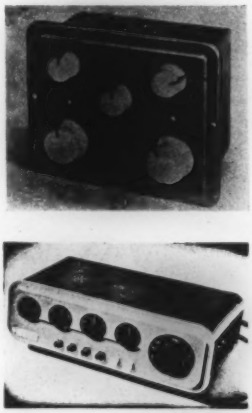
ability to give such instruments even the possibility of material existence.

If a scientific instrument may be defined as an object used as an aid in scientific study, or in the application of science, then the possible range covered by the term is vast. It will include the surgeon's scalpel, the computor, the heart-and-lung machine, the bomb-sight; a sputnik and a radio-telescope. Also, the motor car speedometer, the kitchen weighing scales, the gas meter,

the radio set, the telephone and controls on the domestic electric cooker. One might equally regard a battleship and a pair of spectacles as scientific instruments. For my present purpose I intend to confine my lecture to those scientific instruments where there is some direct link with their users; that is those instruments with which we are in direct contact, either to give us information or to enable us to control machines or other apparatus, and which affect us directly by their design because we use them, or because they form part of our living and working environment.

By " design " we mean the previous planning and definition which takes place and which gives the possibility of existence to anything before it can have material existence. Generally speaking it is a process of the imagination whereby a wide range of information —mechanical; economic; structural; possible processes of manufacture; economic; aesthetic; functional; physical; chemical; psychological; etc.—is synthesized. If any one or more of these factors is missing from the design, or if insufficiently considered, the eventual effectiveness of the final product is correspondingly weakened. For example, inadequate knowledge of ultimate working stresses may lead to structural breakdown whilst a product is in operation. Also a designer who lacks knowledge of posture and human physiology may produce mechanically efficient equipment which is both difficult and fatiguing to operate. From this it can be seen that the process of design, which is the fusion of all knowledge relevant to the purposes, manufacture and function of products, is to-day made difficult by the exclusiveness of the separate branches of this knowledge. It is possible for the designer to be unaware of knowledge which already exists, and which could have a direct bearing on his design. This can sometimes be due to ignorance, but also it may be due to the fact that knowledge can be inaccessibly buried in remote specialisms. In the case of the design of scientific instruments, the main disadvantages resulting from the possible lack of consideration of essential design factors come from the fact that in a scientific and technical age, the purely mechanistic view has either predominated over the aesthetic, or ignored it altogether.

What do we mean by " aesthetics " ? The word frightens many scientists and most technologists. The reason for their occasional reaction in ridiculing the subject is much the same as the ignorance which prompted current opinion to deride Galvani as " the frogs " dancing master ! It may also be due to the same powerful influences and beliefs which made the protests of nineteenth century aesthetes against the slums, squalor, drudgery and filth of Victorian industrial towns so ineffectual and hopeless—the entrenched belief of the Establishment that the sacrifice of life, health, enjoyment and a wholesome environment can be sanctioned and excused if it brings in a commercially profitable return. " Aesthetics " is a field of study concerned with the reactions of the senses and brain. It is the science or exact study of perception. It deals with the way in which we react to sensory experience. Unfortunately since Baumgarten wrote his philosophical treatise on the nature of beauty (Frankfurt 1750-58), and used the word " aesthetic " in connection with this, we have tended to confuse aesthetics with beauty: whereas the aesthetic reaction to beauty is only one of our possible reactions when we perceive objects and experience sounds, tastes, etc. The reactions of our many senses can range from acute pain, through discomfort, irritation and indifference, to those perceptions which are tolerable, likeable, pleasant and very pleasant; ultimately to the experience of great beauty itself. These reactions may be experienced in relation to natural phenomena, such as to the forms, scents and colours of flowers, and to the colours and shapes of crystal formations. They may also be experienced in connection



(*a*)

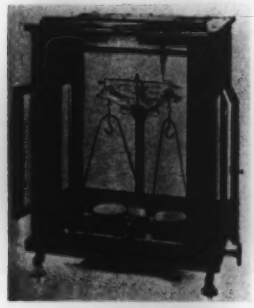
(*b*)

Fig. 4. *The legibility and orderliness of* (*b*) *contrasts with similar model* (*a*) *where little thought has been given to good appearance and the convenience of an operator.*

with man-made objects. For example, if we drive a motor car, we can not only see if the producer has taken pains so that its appearance is pleasing, we also notice if he and the road engineer have created the necessary conditions to give us a smooth ride with an absence of unpleasantly noisy vibration. Here, as in other cases, no argument is necessary to convince you that you prefer your aesthetic reactions to be pleasurable rather than the reverse. Your feelings are an adequate guide in this. As I have pointed out it is often possible to have a pleasurable aesthetic response to natural phenomena where no human intervention at all is involved. These same reactions can be experienced also to man-made products, such as to certain aerodynamic forms, to the beauty of the turbine blade, where there has been no conscious effort made to make them look pleasing. On the other hand, where there is a conscious aim to make products expressive of beauty, this lies within the sphere of the artist, whether he is recognized as a specialist artist designer, or whether we use the term to describe the " artist " in every person. The professional

artist is, of course, concerned to express a wider range of emotional experience than the reaction to beauty, but nevertheless it is an important part of his work. Also, since he necessarily works through the medium of our aesthetic perceptions, he has great professional expertise in the many ways of doing this. It is for this reason that the finest actors have clear diction, that the finest artist typographers produce the most beautiful and legible letter forms: and that the most skilful furniture designers produce chairs which are a joy both to look at and to sit on. It is this same aesthetic concern which can make the disposition and sequence of controls on an instrument display panel easy to follow and understand, make a scientific instrument, used to record and give information, easily and clearly legible in use, and result in hand controls being dimensioned and placed in such a way that they are comfortably convenient to an operator.

The place of aesthetics in the design of scientific instruments is to " humanize " them. For example, to give them beauty or harmony if they lack these qualities: and to make them appropriate to the characteristics of those of our senses and feelings which are called into play when we use or operate such instruments. When we include this aspect of design, we usually do so for three broad reasons. First, because it is a natural human tendency—only denied by maladjusted barbarians, philistines and sadists—to try to make the human environment as pleasant and wholesome as possible; whether it is the living-room, the commercial office, the work's canteen or the scientific laboratory. Second, because when a selection is made from a number of



(*a*)

(*b*)

Fig. 5. *The Laboratory balance, for so long designed almost solely with mechanical accuracy in view (a) is now being progressively considered from an aesthetic point of view as well (b).*

(*a*)

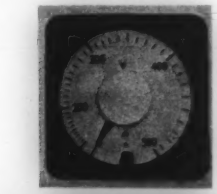
(*b*)

Fig. 6. *The design of the pointer knob in (a) ignores one of the basic principles of visual discrimination—that good visibility depends on contrasts of dark and light tones. The pointer in (b) is much more easily perceived.*

similar products within a given price-range, the final choice is usually made on grounds of quality: that is on appearance and performance. Products which are convenient to operate and of good appearance are generally more commercially successful than those where these factors are neglected. Therefore the aesthetic consideration in the design of products often has a commercial motive—particularly if the market concerned is highly competitive. Third, for the reason that in prosperous, civilized and highly industrialized societies, operators demand industrial equipment and apparatus which is designed with their operational convenience in view; and also because managements usually find it economically wise to use equipment which is pleasant for operators to use, and which is safe, efficient and comfortably convenient from a human point of view—where men can master machines, instead of the reverse.

If we decide to include aesthetics in design, what can we do to achieve this successfully ? Any attempt to make products visually beautiful have usually followed four courses. First, by the use of *decoration*. That is, for example, where, by some mysterious process a form is considered more pleasant if it is decorated with a

pictorial representation of something, like a plant form, which happens to be beautiful. This can be seen in the way domestic dinner services are decorated with patterns and other ornament. It is hardly a suitable aesthetic design treatment for the present manufacturing processes of scientific instruments, though early chronometers, telescopes, barometers, etc., were often embellished with either engraved or painted decoration, in the same way that the barrels of muskets and cannon were enriched with ornament. Second, by the use of *figurative* treatments. Here, the shape of one product is made in the form of some other entirely different



(*a*)

(*b*)

Fig. 7. *Meters designed for industrial use (a) are usually aesthetically considered so as to be legible in use. On the other hand, meters used in mass production motor cars are sometimes stylistically treated in the "science-fiction" manner.*

product which is considered to be beautiful. This approach usually takes the following course: "Greek pots are considered to be beautiful. Therefore if we make miniature Greek pots, and put the working parts of a cigarette lighter inside them, we will, in this way, make beautiful cigarette lighters." This figurative approach explains why a city like Manchester—and many other cities built when the figurative approach was rife—can have a make-believe "Roman" Reference Library, a mock "Gothic" Town Hall, a "Greek" porticoed Art Gallery, and several pseudo "Tudor" public houses. This figurative approach to design has often been used in the aesthetic design of scientific instruments, for Greek inspired mouldings, pillars, acanthus decoration and classical figures have been used on Post Office weighing scales, early microscopes (in the elegant scientific instruments produced in the eighteenth century by George Adams), the structural supports of early electrical apparatus, the telephone, etc. Third, by the approach of modern *abstract* art and design, which takes the view that forms should be beautiful in themselves in their shapes, colours, textures and lines, irrespective of decoration or figurative treatments. Also that such forms of products should primarily be the outcome of the functions which they are to perform, and their suitability for the materials in, and manufacturing processes by, which they are made. There is nothing new about the existence of abstract art; for example, many of the most beautiful thirteenth century Chinese pottery forms are plain undecorated shapes which owe their beauty to the refinement and elegance of their shapes, colours, outlines, and beauty of surface and material. This latter, the abstract, is the most suitable visual aesthetic treatment to-day for scientific instruments.

There are, however, senses other than the visual through which we feel aesthetic reactions. Smell, touch, sound, ease of physical movement and mental comprehension, reaction to temperature, awareness of atmospheric pressure, vibration and balance. Many of the "human" sciences, such as psychology, physiology, neurology and biometry, are sources of information which the designer requires to enable him to design with these other senses in view. Anthropometry—the study of human proportions and dimensions—can enable instruments to be conveniently arranged for human use, so that, for example, dials and controls are in positions and on levels where they can be easily seen and operated. Ergonomics—the study of the relationship between man and equipment of all kinds—is the combined field of applied psychology, physiology, and neurology. It can aid the designer to plan instruments so that they are easily understood, related to efficient human movement and good posture in the performance of tasks. From these fields of study, information may be obtained to enable the designer to create and shape equipment so that it is related to the fundamental organic nature of the human beings who operate it, so that elements—unfortunately only too common in much industrial equipment used in the recent past—are eliminated which cause discomfort, fatigue and even premature ageing. In accepting the basically organic nature of man, they oppose the view that man should adapt himself to machines, and hold that all products and machines should be designed in harmony with mans' senses, physical nature and mental needs—a point of view which received scant attention in nineteenth century industrial England.

Other ways of making scientific instruments pleasing to prospective users might be described generally as those which use methods of psychological persuasion. One of these is by stylistically treating products so that they appear to be better than they really are: certain

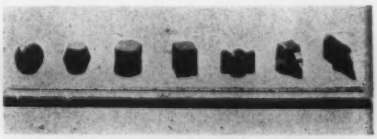


Fig. 8. *Aircraft control knobs designed to be identified by the sense of touch.*

glossy, chromium decorated motor car styling treatments, with their exaggerated aero-dynamic " fins," are examples of this. Another may be described as " science-fiction " styling, where products are designed to resemble forms which have resulted from scientific developments which in their turn have gripped the popular imagination. For example, it may be seen in the use of aero-dynamic forms for quite static objects, the widespread use of the shape of the television screen for speedometers, ventilators, etc., and the use of shapes derived from rockets for some motorcycle and motor car body designs. Many of these pseudo-scientific design treatments may often be seen applied to instrument controls on a wide range of consumer products such as: cars, radio and television sets; electric cookers; small power tools; clocks, cameras and slide projectors—apparently with great commercial success, though sometimes diminishing the efficiency with which such instruments should be used.

In addition to the present divergence between art and science, there is the added confusion that both the artist and the technologist are asked to design and produce scientific instruments from two points of view. One, to design them to suit popular mass taste, so that large sales will sustain commercial mass production—as in the case of many speedometers and fascia control panels used in mass produced motor cars. The other is to concentrate on designing and producing a plain, accurate and reliable instrument for specialist industrial or scientific use. Both aims are sometimes confused with each other; and both at present frequently omit to take into consideration that it is not sufficient to guarantee a high mechanical, and electrical performance of, for instance, automation controls, and controls for supersonic air liners. These, and all scientific instruments which are directly used by human beings can only be judged efficient in performance if they work well in conjunction with the people who have to use them—the aesthetic aspect of design aims to make a contribution precisely in that direction.

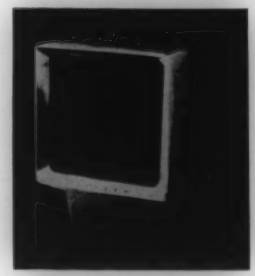


Fig. 9. *The " Vistavu." A sensitively styled slide viewer produced by the Rank Precision Industries Limited. Winner of a Council of Industrial Design Prize for its good appearance and operational convenience 1958.*

*Vol. 11, May, 1960*

# The work of the Institute's Electronics Group

C. A. HOGARTH
B.Sc., Ph.D., F.Inst.P.

*Brunel College of Technology; Honorary Secretary of the Electronics Group*

ELECTRONICS as a subject in its own right developed with the rapid advances which occurred as a result of military requirements during the Second World War. In pre-war days, the field of electronics was largely concerned with valves and cathode-ray tubes intended ultimately for the entertainment and communications industries, for certain military applications and for a few scientific instruments. There was little or no hint that twenty-five years later electronic devices would be used in many walks of life, in the process-control systems of major industrial undertakings, in new types of astronomical telescope, and in general for replacing mechanical devices in countless applications.

In 1939 electronic devices included valves and cathode-ray tubes, very primitive photo-conductive devices, metal rectifiers, primitive crystal detectors and microwave generators, and phosphors of not very reproducible properties. At that time electronics really formed part of the electrical engineering industry and much of the research effort was on novel circuits and systems using conventional valves. Small research groups in various parts of the world were pursuing physical investigations but the magical possibilities so soon to be exploited fully were little more than possibilities or in some cases feasibilities. Much of the work on phosphors was concerned with applications to television systems, a small amount of work on microwave generation and detection was related to military needs for the early radar systems, and the properties and uses of metal rectifiers were being actively explored. Although a famous physicist had been responsible for the discovery of the electron in 1897, and the father of nuclear physics had worked as a research student on communications systems, the major advances in electronics in pre-war days arose largely from the efforts of engineers.

With the military needs brought about by the technical nature of many of the operational systems used in the war, a great effort was put into the study of the physical properties of existing and novel electronic devices. Microwave generators, notably the cavity magnetron and high-power klystron were investigated rapidly and carefully, the silicon microwave detector and mixer was developed to become a reliable component, T.R. switches of novel type were invented, metal rectifiers and later germanium point-contact diodes were developed, photo-conductive cells for infra-red detection, image-converter tubes and skiatron tubes for display equipment were investigated scientifically, and even conventional valves were re-designed in smaller sizes and to have vastly increased reliability. For investigation of the above matters the physicist was needed since the formal training of electrical engineers then included but little of the physical background to the mode of operation of the new devices.

During the war the Electronics Group of The Institute of Physics was formed to provide a forum for those interested in electronic physics, physical electronics, or some other permutation of these or similar words. In the constitution of the Group the scope of its interests is defined as "any matter relating to the physics and applications of electronics (except X-ray) devices." The objects of the Group are:—

(*a*) To hold meetings of members either independently or jointly with other bodies, for the reading and discussion of papers, for the delivery of lectures and for the discussion generally of subjects coming within the scope of the Group.

(*b*) To arrange visits to works, research establishments, etc., calculated to be of benefit to members.

(*c*) To stimulate interest in the subjects falling within the scope of the Group.

(*d*) Generally, to advance the science and technology of the subjects within the scope of the Group.

The general practice has been to implement the objects listed above in terms of the suggestions of members and Committee. The operation of complicated electronic circuits or systems has not normally been the concern of the Group except in so far as this sort of subject illustrates the mode of operation or physical significance of a particular device or devices.

The management of the Group is in the hands of a Committee consisting of three Officers (namely, a Chairman, a Vice-Chairman, and a Honorary Secretary, who also acts as Honorary Treasurer), together with not more than seven other members. A Fellow of The Physical Society sits on the Committee by invitation. This arrangement helps to ensure that there is no major conflict over the choice of subjects for meetings and conferences. The Officers of the Group are elected annually (in March) and the Chairman is not allowed to serve continuously for more than four years. Two committee members retire annually. The Group receives from the Institute an annual grant which covers its expenses.

The general programme of the Group's activities is arranged in terms of the following pattern:—

(*a*) Six lectures (preceded by an informal tea) at monthly intervals during the months October to March, normally at the Institute's House.

(*b*) An Autumn Meeting on some special theme, often held at a provincial centre. This meeting normally lasts about 1½ days and although novel work is usually presented the opportunity is taken to invite specialists to give accounts of recent advances in their own relevant fields.

(*c*) Occasional visits to firms and research laboratories.
(*d*) Joint meetings with other bodies.
(*e*) Major conferences.

Since the formation of the Group in 1942 there has been an ever-increasing activity in the discovery and development of new electronic devices, many of an increasingly perplexing mode of operation. Possibly the most notable was the invention of the transistor in 1948 and the subsequent development of many *p-n* junction devices. The understanding of the action of these devices involved the use of fairly subtle concepts of the physics of the solid state and the study of solids has since become increasingly important. The Group still has an interest in high vacuum physics as relevant to thermionic devices but an increasing emphasis in solid-state physics has been noted over recent years. Such topics as thermoelectric devices, microwave ferrites, thermistors, masers and parametric amplifiers, single crystal photo-cells, and solid-state switches have widened the 1942 interests of the Group enormously.

From time to time suggestions are made to the effect that the name of the Group should be changed but a better alternative has not yet been suggested. A new informal definition of the scope of the Group was attempted in November 1956, by Dr. D. A. Wright and Mr. L. A. Thomas who proposed the following:—

" The Group may interest itself in any aspect of the physics of electronic devices. This branch of physics is very wide and includes for example:—

Electron Emission
Electron Optics
Vacuum Physics
Gas Discharges
Semiconductors
Luminescence and Photoconductivity

and the appropriate aspects of magnetism, dielectrics and ferroelectricity."

It is still possible to select suitable topics which do not quite fit the exact headings above but for practical purposes this statement serves as a working guide.

Some illustrations of particular activities of the Group are now given so that their topicality and general value in promoting an interest in, and aiding in the dissemination of information on, the physical background of electronics may be observed. As examples of (*a*) above, twelve titles of recent lectures to the Group are listed:—

Crossed-field interaction in microwave valves
Electroluminescence
Electrical analogues
Microwave ferrites
Information theory
The luminescence of solids in the infra-red region
Čerenkov radiation
Germanium and silicon crystals and devices
High vacua
The effects of radiation damage on the properties of solids
Display tubes for colour television
The electronic theory of valency

Attendance figures at these meetings generally lie between 30 and 100. The Secretary of the Institute frequently invites lecturers at particularly successful meetings to prepare a special article, based on the lecture, for one of the Institute's journals.

Under (*b*) recent autumn meetings have been held as follows:—

Sept., 1956 " The Properties of Thin Films " (Held in the University of Reading and attended by about 80).
Sept., 1957 " The Physics of Transistors " (Held at Brunel College and attended by about 200).
Sept., 1958 " Solid-state Memory and Switching Devices " (Held at University College, London and attended by about 200.)
Oct., 1959 " Noise in Electronic Devices " (Held at Services Electronics Research Laboratory, Baldock, and attended by about 100.)

Few special Group visits to research laboratories and firms have been made in the last few years partly because opportunities for such visits have normally been combined with a conference or autumn meeting. If the Committee receives any requests for the frequency of such visits to be increased it will consider them when preparing the future programme.

Joint meetings with other bodies both within and outside the Institute are held from time to time. A major conference on " Solid-state Microwave Amplifiers " has been arranged in conjunction with the Radio Spectroscopy Group and will be held in April, 1960, at the University of Nottingham. In April, 1958, a popular one-day meeting on " Solid-state and Data Handling " was held at Malvern, in association with the Midland Branch of the Institute. At present, preliminary discussions are under way with The Physical Society and the Solid-state Commission of the International Union of Pure and Applied Physics with a view to arranging an International Conference on Semiconductors in the U.K. during 1962. In May, 1959, the Institution of Electrical Engineers held an International Convention on " Transistors and Associated Semi-Conductor Devices " in London. The Honorary Secretary of the Group (not a member of the I.E.E.) was invited to serve on the Organizing Committee and the Technical Programme Sub-Committee. He expected this to represent simply a courteous gesture to attend Committee Meetings. He was much mistaken in this as he found himself " Co-ordinator of Basic Theory " and was expected to work as hard as did the other I.E.E. Committee Members. He was invited to chair one of the sessions of the Convention. This invited co-operation with the I.E.E. was much appreciated by the Group.

In 1954 a conference on " Luminescence " was held at Cambridge and attended by scientists from many countries. The proceedings of the conference are still used as a work of reference to the subject.

The above examples indicate how the Group Committee endeavours to provide a varied and interesting program of activities for members and guests and to meet the " objects " of the Group outlined above. The Committee receives very little guidance from members in terms of requests for particular activities and would appreciate suggestions from members for consideration. Although attempts are made to treat the many developing lines which constitute " electronics," no Committee

is perfect and suggestions from other members are therefore both valuable and welcome. It is very rarely that an invited speaker fails to reply favourably to an invitation. Indeed in 1958, 1959, and 1960, at least one distinguished foreign visitor to the U.K. addressed or is scheduled to address the Group. In discussing further activities the Committee has decided that meetings of a predominantly social nature are properly the province of the Branches of the Institute although meetings of this nature may be planned in association with a particular branch. It has also been suggested that " Discussion " meetings should be held.

No brief account of the work of the Electronics Group would be complete without a reference to the support given to the Committee by the permanent officers of the Institute and the staff, who make sure for example, that notices of meetings are posted, tea arrives on time, slides are shown efficiently, and money is made available. Without their untiring efforts on behalf of the Group, the Secretary's lot would be " not a happy one."

# Notes and News

*Professor C. F. Powell (left) and Professor A. M. Tyndall at the South-Western Branch lectures for school children.*

**South-Western Branch : Lectures for schoolchildren**

The South-Western Branch of the Institute recently arranged two lectures for sixth-formers, given by Professor A. M. Tyndall, F.R.S., and Professor C. F. Powell, F.R.S. The lectures were given on the afternoon of Monday, April 4, and repeated on the following day. The lecture theatre was full to capacity (380) on both days and so great was the demand for tickets that it could easily have been filled a third time, had this been possible. Professor Tyndall took as his subject " The Atmosphere as a Conductor of Electricity " and dealt with basic phenomena of ionization, etc. Professor Powell followed with an account of the application of these ideas to the detection of fast particles, and so led up to a discussion of " Cosmic Rays "—the title of his talk.

The venture was generally felt to have been very successful, and it is hoped that it will be possible to make it an annual event, timed to take place at the end of the Spring term.

**Textile Institute, Fiftieth Anniversary**

The following address was presented to The Textile Institute on the occasion of its golden jubilee :

" The Institute of Physics presents its congratulations to The Textile Institute on the occasion of the celebration of its fiftieth anniversary.

In your first fifty years you have become the world's leading professional organization for textile technologists, and your high professional and scientific status is now recognized throughout the world wherever there is a textile industry.

Of all branches of science that can influence the future of the textile industry none is more important than physics. Your recognition of this is shown in many ways, and recently in the formation of a Physics Group within the Institute—a step we greatly welcome.

In the vital field of education you have played and are playing a great part. The standard of your *Transactions*, which are largely applied physics, your conferences, your text-books, your comprehensive abstracts of world wide repute, and your scholarships, are all indicative of the outstanding contribution your Institute is making to the problem of education for industry.

Within your membership there are many Fellows and Associates of The Institute of Physics who, now holding your diplomas also, have become textile technologists of distinction. The ever increasing impact of science on the textile industry will continue to strengthen this link of common membership between us.

It is a pleasure to convey to you, on behalf of The Institute of Physics, our good wishes for your continual progress in the great work you are doing.

G. P. THOMSON, *President*."

**Physics Group of The Textile Institute**

The Physics Group of The Textile Institute will meet at the Cairn Hydro Hotel in Harrogate on September 22 and 23. The program will include reading and discussion of nine papers, the titles and authors of which are as follows :—" The Mechanics of Small Fabric Deformations, with Particular Reference to Bending," by D. N. E. Cooper ; " Shearing, Bending, and Drape of Fabrics," by G. E. Cusick ; " Assessment of Fabric Handle," by W. S. Howorth ; " Fabric Creasing and the Viscoelastic Properties of Fibres," by Mrs. A. M. Rees ; " The Geometry of Fabric in its Relaxed Condition," by D. L. Munden ; " Weaving Resistance —its Nature and its Effect on Fabric Construction," by K. W. Greenwood ; " Polarizing Lustre Meter," by P. S. H. Henry ; " Colour Effects in Blend Fabrics," by J. C. Guthrie and P. H. Oliver ; " The Role of Cloth Geometry in Fabrics for Coating," by V. K. Hewinson. Members of The Institute of Physics will be welcome at this Conference. They should write for further details to The Textile Institute, 10 Blackfriars Street, Manchester 3.

**Addresses wanted**

The Institute no longer has effective addresses for the members named below. Any members who can supply current addresses are asked to notify the Secretary.

| *Name* | *Last known location* |
|---|---|
| ASSOCIATES | |
| S. W. Cousins | Surbiton, Surrey |
| P. L. Flowerday | Cleethorpes, Lincs. |
| C. A. K. Salgado | Handsworth, Birmingham 21 |
| J. W. Watts | Kuala, Malaya |
| D. Wilkie | Holmrook, Cumberland |
| GRADUATES | |
| A. G. Caddick | Rochester, Kent |
| D. G. Carter | Maidstone, Kent |
| P. J. Fowler | Nuneaton, Warks. |
| H. Gurudas | Wallington, Surrey |
| E. Smart | Tynemouth |
| A. E. Sparrow | Wells, Somerset |
| J. H. Wilson | Aylesbury |
| P. Wood | Stevenage, Herts |

*Vol. 11, May, 1960*

# Reviews of Books

## CAUSALITY—THE PLACE OF THE CAUSAL PRINCIPLE IN MODERN SCIENCE

**By Mario Bunge**

*Massachusetts : Harvard University Press, London ; Oxford University Press. Pp. xx + 380. Price 60s.*

The thesis of this book is that causality is one of several forms of determinism. The principle of determinism, which is " a philosophical assumption of science," is that " everything is determined in accordance with laws by something else." Causality is more specific : it is the doctrine that " if C happens, then (and only then) E is always produced by it." The view is advanced that while causality, unlike determinism, is not necessary to science, neither is it forbidden ; it is approximately valid in certain fields.

The treatment is clear, thorough, and systematically presented. The terms having been defined, a statement is given of what causal determinism does and does not assert, and its place in science is examined from various points of view. Physics claims the lion's share of the discussion, but the social sciences also receive attention, biology scarcely any. It would be impossible in a short space to give an adequate idea of so minutely reasoned an essay. All that can profitably be done here is to indicate one or two presuppositions of the book which have prevented the reviewer from giving full assent to its claims.

There is what may be called an excess of absolutism running through the work which results in the presentation of what are ultimately matters of free choice as matters of objective fact. What is absolute in science is experience, observation : that must be accepted precisely as it is, neither distorted nor explained away. But the concepts used to describe it are at our choice, and whether a law is causal or not often depends on that choice and not on the nature of the underlying experience. For example, Professor Bunge claims that the Newtonian law of gravitation, usually regarded as causal, is " typically *non*-causal," because it describes an *interaction* between two masses, not an action of one on the other. In a special case—e.g. a stone falling to the Earth—one mass is so much greater than the other that we can say that it *caused* the acceleration of the other, but that is only an approximation. But in fact what we are given is the simple situation, not two independent masses ; they are the terms we choose in order to express the situation. We can equally well express it in terms of force and motion, and then we can say that the force causes the motion, whatever the relative sizes of the bodies. Both force and motion are functions of the *pair* of masses taken together and their individual values do not enter into the description. The law then becomes purely causal.

More generally, it is argued that causality is inapplicable to circumstances in which there are necessary interactions—reactions accompanying actions—because causality necessarily implies a one-way process, from cause to effect. But interaction does not exclude one-wayness. A acts on B and B reacts on A, but the interaction itself is one-way ; it is an attraction and not a repulsion. We do not in physics conceive of situations in such a way that A attracts B while B repels A : that liberty has up to now been the prerogative of novelists. But it is not prohibited to physicists. If A and B are regarded as *necessary* elements in terms of which the situation must be expressed, then this is a distinct possibility. If, however, the same situation is described in terms only of the interaction, it is superfluous ; the *resultant* interaction is all that is significant. So when Professor Bunge says that " in reality most reciprocal actions are not symmetrical," his " in reality " betrays the false absoluteness to which I have referred. Gravitation is a reciprocal action ; it is symmetrical as between A and B and unsymmetrical as between attraction and repulsion. What has " reality " got to do with it ?

There are other examples of the same preconception. Thus, the distinction drawn between " internal " and " external " causes supposes that these are absolutely distinguishable. But they are matters of voluntary definition. Is the influence of the " unconscious " on one's actions internal or external ? It depends entirely on whether one chooses to define the self solely in terms of consciousness or not. We can take our choice ; the actions are the same in either case. The concept of the self is not given us ; we form and define it as we like. And when Professor Bunge, commenting on the discrepancy between Aristotle's " real " universe of spheres and Ptolemy's calculating device of epicycles, remarks that " nobody seems to have drawn the correct conclusion, namely, that a scientific theory need not be causal," the word " correct " again reveals the same prejudice. Whatever meaning attaches to " scientific theory " belongs to it by our voluntary choice. It may or may not be fixed now by custom, but it is surely a little hard on writers in the twelfth century, who are here under criticism, to expect them to have foreseen what our custom would be.

Another fault—which, alas, is almost universal—is lack of the historical sense, of the realization that the theories now current in physics will probably appear in the year 2060 (allowing for the acceleration in progress) as those of the year 1760 do to us. They are, as usual, taken here to be established truth. " Special relativity (1905) finally banished instantaneous links from mechanics. . . ." Special relativity is a theory admitted by its author to depend on two assumptions and a voluntary definition, but for some totally unexplained reason it has now become " final," though the assumptions still remain assumptions. Again, there is a reference to " the (impossible) attempt to frame thermodynamics in a causal language." Why impossible ? The utmost that can be said is that at the moment we prefer another language ; the attempt in question has not been, and cannot be, proved impossible. Why is this particular epoch so singularly blessed above all previous ones ?

It is easier and more interesting to discuss shortcomings than to enumerate the valuable features of a book, but that should not be held to imply that the latter do not

exist. As a stimulus to fruitful thought this volume has great merit and it can be warmly recommended to all who wish to clarify their ideas on the subject of which it treats. It shows wide reading, and although the reviewer, in self-protection, is bound to say that the two references in it to himself are both incorrect, he has no reason to believe that this denotes general carelessness.

HERBERT DINGLE.

## LOW-TEMPERATURE TECHNIQUES

**By F. Din and A. H. Cockett**

*London: George Newnes Ltd. Pp. viii + 216. Price 40s.*

The book under review is a recent addition to the series of practical science books published by Geo. Newnes. It provides a source of up-to-date information about the principles and practice of low temperature techniques. The emphasis is on the practical aspects and mathematical symbolism has been kept to a minimum. The book will be invaluable to those embarking on research involving low temperatures.

The authors briefly sketch the work of the early pioneers in the production of low temperatures notably Olszewski and Wroblewski in Poland from 1880 to the end of the century. Kamerlingh Onnes in Holland where helium was liquefied in 1908 and superconductivity discovered in 1911.

The development of helium liquefiers by Collins in the U.S.A. has resulted in a world-wide spread of facilities for low temperature research.

For temperatures below 1°K resort is made to the technique of cooling by adiabatic demagnetization.

The measurement of low temperatures is dealt with adequately, the thermodynamic ideas underlying the temperature scales are explained and practical gas thermometry dealt with. With the platinum thermometer it is now possible to make temperature measurements to within about 0·03° of the absolute scale in the range 0°C to −183°C. Between the boiling point of hydrogen and about 5°K temperatures can be measured satisfactorily only by means of a helium gas thermometer or some secondary device that has been compared with one. There are, however, some fixed points between the boiling point of helium and that of oxygen which can be used to calibrate secondary thermometers.

Descriptions are given of vapour pressure thermometers and of industrial gas thermometers. Mention is made of a thermometer designed to cover the range from about 3°K to 300°K. It consists of a Bourdon gauge connected by capillary tubing to a thermometer bulb, the whole being filled with helium. Below the critical temperature of helium, 5·25°K it acts as a vapour pressure thermometer the maximum pressure being 2·26 atm, and at higher temperatures as a gas thermometer, the pressure then depending on the relative volumes of the bulb and the Bourdon tube and the mass of gas contained.

Recent developments in the design of cryostats are discussed and a table given of the freezing points of a number of common organic liquids suitable for use in constant temperature baths in the range 0°C to −120°C. When designing apparatus for the study of the physical properties of materals over a wide range of temperature the great change with temperature of certain properties has to be taken into account. To illustrate this the case of the electrical resistivity of gold may be cited—value at 4°K is only about one thousandth of the value at 0°C.

Useful information is provided as to the mechanical properties of metals at low temperatures. This is of value to the aeronautical industry; the structure of a plane may at times be subjected to temperatures as low as −80°C.

The thermal conductivity of some insulants is also discussed. The student will find the account of the theoretical studies of the variation of the thermal conductivity of metals with temperature particularly instructive.

The information as to the early work on the atomic heats of the metals which played such an important part in the establishment of the quantum theory is also to be commended. There is given a clear account of the properties of the two isotopes of helium. A chapter is devoted to gas separation. The various methods are concisely discussed including the diffusion process for the separation of the isotopes of uranium for atomic energy purposes, the volatile compound involved being the hexafluoride.

Separation of mixed gases by distillation is well described and includes an account of the isolation of the heavy hydrogen isotope, deuterium, and the production of tonnage oxygen. The concluding chapter of the book deals with insulation, storage and transport of liquefied gases, industrial refrigeration down to −100°C, uses of liquefied gases.

The information on fibrous and powder insulants and on vacuum insulation embodies the experience of the authors in the storage and transport of liquefied gases which includes transmission in pipelines.

The information contained in this book will be helpful to both the student and the technologist.

EZER GRIFFITHS.

## THEORY OF A.C. CIRCUITS

**By Sylvan Fich and James L. Potter**

*London : Macmillan & Co. Ltd. Pp. xii + 453. Price 35s.*

The treatment of alternating currents in physics courses varies considerably. In some cases only very simple circuits are considered and these are analysed by means of the appropriate differential equations. Other courses include some advanced a.c. circuit analysis. To-day there would seem to be no excuse for any physics graduate not having some knowledge of the powerful symbolic method of analysis of steady state a.c. circuits. At the same time it is most important that the limitations of the " a.c. Ohm's Law " should be fully appreciated. The book under review provides a very good introduction to the symbolic method. Although it assumes a knowledge of the principles of electromagnetism, it starts right at the beginning of alternating currents. After an introductory chapter, vector or phasor algebra and the use of complex numbers are discussed fully and justified thoroughly. The theory is applied in turn to

simple L C R circuits, resonance, general network analysis, coupled circuits and polyphase circuits. The book concludes with a chapter on circuit synthesis. The approach is essentially mathematical throughout but all the necessary mathematics is derived as required. Complex algebra, Maclaurin's Theorem, exponentials, hyperbolic functions, determinants and matrices, Fourier analysis and Laplace Transforms are all introduced and their treatment is clear and sufficiently rigorous. There is a collection of examples (with answers) at the end of each chapter.

Although the penultimate chapter deals with transients most of the book is limited to the steady state. This is perhaps unfortunate since it is highly desirable that students should come to grips with transients at an early stage. With this stricture the book can be thoroughly recommended, provided it is supplemented by a sound physical treatment of electromagnetism.

M. R. Gavin.

## PHOTOMICROGRAPHY

**By R. M. Allen**

*London : D. Van Nostrand Company Ltd. Pp. xiii + 441. Price 67s. 6d.*

This is the second edition of a book first published in 1941 and twice re-printed. It has proved, therefore, that it has filled a need. The reason, undoubtedly, is that it is a pleasantly written and well illustrated " How to do it " book. Since this review appears in a journal for physicists it is as well to say right away that a physicist may well smile at a few places in the book, for example, on reading of " the rainbow, which is caused by sunlight passing through the raindrops, each of which functions as a minute spherical lens " or noting that, in the schematic Figure 9, because the eyepiece of a microscope is represented by a single lens, the rays in the diagram have to suffer an abrupt and inexplicable change of direction, at the location of the image formed by the objective, in order to get them to come correctly out of the eyepiece. A chance was missed here to explain the function of what is essentially the field lens in Ramsden and Huygens eyepieces. I get the impression that the author is not quite sure of his theoretical optics nor of the more theoretical aspects of photography. On the other hand, again for example, the description of the way in which transparent objects are rendered visible by phase contrast has some attractive features, particularly in its insistence on the similarities of the phase contrast method and older methods such as dark-ground illumination and stopping down the condenser and defocusing. Photomicrographs of the kind used by Dr. Allen in this discussion could well be used to ornament and add to the intelligibility of more abstruse accounts of the theory of the microscope. The writer of a book of this kind is, of course, in trouble about theory. For, although a proper understanding of theory is usually helpful in practice (not always : would a detailed knowledge of the theoretical dynamics of a bicycle help anyone to ride it ?) it is not easy to describe theory accurately and yet with the brevity and intelligibility which would make it attractive to most of us. Dr. Allen may not have offered us quite enough theory to make the optics of the microscope truly intelligible, but at least he has not made the mistake of offering too much. The photomicrographs illustrating theory will carry conviction to the practical man where theory itself might not.

The publishers say, " this practical book brings to the professional a broad, accurate picture of modern photomicrography—to the hobbyist or beginner, a unique, simply-written guide to a fascinating field." Allowing just a little for the natural enthusiasm of publishers this is a fair description. I find some small features attractive. First, the story of the author's first and successful attempt, as a lad, at photomicrography by balancing a box-camera on the eye-piece and flipping the substage mirror so that a beam of sunlight passed suddenly through the slide. Second, the inclusion of a chapter on home-made equipment, and the statement of one of the reasons for home-made equipment, namely, " for the joy of making it." Third, the appearance in the most important chapter of a little section on " Pictorial Composition in Photomicrography." This chapter, on " The Technique of Photomicrography," rightly takes up a quarter of the book. It is full of good sound advice.

The book was printed in the U.S.A. ; it is well produced, with 235 illustrations, mostly half-tones, in the text and 54 full-page plates, of photomicrographs, all on paper that is, I think, slightly less shiny than this *Bulletin* uses.

E. W. H. Selwyn.

## PHYSICS OF METEOR FLIGHT IN THE ATMOSPHERE

**By Ernst J. Öpik**

*London : Interscience Publishers Ltd. Pp. viii + 174. Price 30s.*

The entry of fast moving bodies into the upper atmosphere has probably been extensively studied in recent years, and it may be that more is known than is openly published. In reading this monograph by Dr. Öpik some physicists may realize for the first time the extraordinary complexity of the phenomena involved. To quote from the introductory chapter, " — factors numerically evaluated [include] aerodynamic drag, coefficient of accommodation, radiation losses, vaporization, fusion, viscosity and spraying of liquid at fusion, stability of liquid drops in flight, role of rotation, aerodynamically induced oscillations and damping of rotation, formation of an air cap, shielding effect of own vapours, heat transfer by conductivity, and impact radiation from vaporized substance." How many of to-day's fashionable problems involve such a wide range of classical physics ?

In 1923 Lindemann and Dobson published in the *Proceedings* of the Royal Society a theoretical treatment of meteor phenomena which played a leading part in establishing interest in and knowledge of the temperature structure of the upper atmosphere. The atmospheric structure deduced from the Lindemann-Dobson theory is qualitatively correct, and the work is cited in modern meteorological texts. Dr. Öpik deals with it in one paragraph, beginning with the words, " Unfortunately it was marred by serious misinterpretations of the laws of

physics." Öpik's own first work in the field appeared in 1922, and the present monograph, written whilst he was Visiting Professor at the University of Maryland, is the result of a lifetime's research. A lifetime's specialization is however not always the best of qualifications for writing a balanced text " to provide a basis for further research," and Dr. Öpik appears at times to discuss the work of others with little more ceremony than he shows to that of Lindemann and Dobson, but with perhaps less cause, and at other times to have an exaggerated regard for his own. There is an amusing example of this on pp. 9-10, where some unpublished observations by Öpik of wind fluctuations over level ground are blandly introduced (quantitatively) into a discussion of turbulent motion at heights of 110 to 140 km. Nevertheless the book is a tour-de-force which will interest, and repay cautious reading by, anyone concerned with meteors, the earth's atmosphere, " space " science, " space " vehicles and, I suggest, the teaching of physics.

G. D. Robinson.

## APPLICATIONS OF THERMO-ELECTRICITY

**By H. J. Goldsmid**

*London : Methuen & Co. Ltd. Pp. xv + 118. Price 10s. 6d.*

In recent years there has been a great deal of interest in the application of thermo-electricity in the form of devices for refrigeration and for the generation of electrical power. These plus the previous uses for temperature measurement and control, for rectification of a.c. for direct reading instruments, and for the detection of radiation, make the foundations of the subject of thermo-electricity of topical interest and industrially important. Dr. Goldsmid's book, therefore, appears at a time when it can be of use (and no doubt will be used) both by students and by research and development workers in the field.

Starting with the usual derivation of the Kelvin relations, the book continues with the development of the theory of thermo-electric devices, thermo-electric properties of semiconductors, figures of merit, selection of semiconductors, alloys, preparation and evaluation of materials, applications of the Peltier effect, and thermo-electric generation. Good bibliographies are included and recent Russian work is well documented. A large amount of useful information is included in the slim but cheap volume, written in a terse but clear style.

To my mind the only criticism concerns the material in Chapter 3 on thermo-electric properties of semiconductors. The author takes several pages to describe the origin of free holes and electrons in semiconductors and then quotes formulae for the thermo-electric power (referred to unambiguously throughout the book as the Seebeck coefficient.) A better use for these pages would be to quote the classical results for hole and electron concentrations and then to include some of the mathematical steps leading to the equations for thermo-electric power.

This criticism apart, the book can be well recommended.

C. A. Hogarth.

## NOTES ON THE QUANTUM THEORY OF ANGULAR MOMENTUM

**By Eugene Feenberg and George Edward Pake**

*London : Oxford University Press. Pp. vii + 55. Price 10s.*

These notes, which introduce and summarize the fundamental ideas on angular momentum in quantum theory, are designed primarily for research students concerned with atomic, nuclear or molecular structure. The opening chapter is a review of quantum mechanical definitions and notation. The concepts of linear operators, matrix elements, commutators and eigenfunctions are introduced and the principles are applied to the example of the harmonic oscillator in one dimension. Angular momentum operators are then discussed in detail and the commutation rules, eigenvalues and matrix elements are deduced. The Pauli spin operators are considered here as an example. Finally the matrix elements of scalar, vector and tensor operators are obtained and applied to several physical problems concerning nuclear magnetic dipole and electric quadrupole moments, quadrupole interactions, transition probabilities and also to a discussion of charge and supermultiplets.

This book gives a brief but very clear introduction to the theory of angular momentum and should provide a good basis for further reading. The short discussion of rotations should also be useful in a later approach to the group theoretical treatment of angular momentum. It is a pity, however, even in so small a book, that there is no mention of Clebsch-Gordan coefficients. A description of the algebra of vector addition of angular momenta would have made it much more valuable.

Daphne F. Jackson.

## NON-LINEAR PROBLEMS IN RANDOM THEORY

**By Norbert Wiener**

*London : Chapman & Hall Ltd. Pp. ix + 131. Price 36s.*

This book of the famous author (the inventor of Cybernetics) is a verbal transcript of 15 lectures given to advanced students of electrical engineering at the Massachusetts Institute of Technology in 1958 on his new mathematical theory of random phenomena in non-linear systems which constitutes an important advance in this field.

A " linear system," e.g. a linear electrical network, has the property that its response to a sinusoidal imput is also sinusoidal, and that the response to a superposition of such imputs is simply the sum of the responses to the separate imputs. Hence it is possible to characterize such a network by an impedance function and, if this is known, to predict the output for any arbitrary non-sinusoidal imput. This does not apply to non-linear systems.

The author suggests to analyse such a system by subjecting it to random imputs, e.g. shot noise, which will result in an output in the form of a random function $F(t, \alpha)$ where $\alpha$ is a random variable, called " phase." This function may be expanded in a series of orthogonal functionals of certain functions $K_n(\tau_n \ldots \tau_n)$ which characterize the response of the system to the simultaneous imputs of $n$ independent random functions $x_n(\tau_n, \alpha)$

If the $K_n$ are known the auto-correlation function of $F$ and hence its spectrum can be computed. Vice versa by suitable analysis of records of random outputs of a non-linear system its characteristic functions $K_n$ can be obtained.

These methods are in particular applied to systems of non-linear random oscillators and the determination of the nature of their spectra. Systems of this type are probably responsible for the electroencephalogram or " electric brain waves " and the theory may also be usefully applied to other problems in physics, astronomy, meteorology and biology.

It is possible to extend the theory from random time series to random functions in ordinary space and in phase space and thus apply it to quantum theory and statistical mechanics.

The book is not easy to read. It assumes that the reader has a fairly advanced knowledge of abstract mathematics, and although the author repeatedly tries to explain in plain language what he is doing, it is often difficult to understand clearly the relationship between his mathematical operations and the physical problems in question. If he had taken the trouble to re-edit the transcript of his lectures these defects could have easily been remedied and the style improved by omitting such phrases as " The next thing is this " or " Are there any other things I can say about $x(t, \alpha)$ ? " etc., which one uses in lectures but sound rather out of place in a book.

R. FURTH.

## ATOMIC ENERGY IN THE SOVIET UNION

**By Arnold Kramish**

*London: Oxford University Press. Pp. x + 232. Price 27s. 6d.*

This book is compiled from a literature search into Soviet publications on nuclear energy, together with some intelligent guesswork on political speeches by Soviet leaders, and extracts from the Soviet press. It outlines the past, present and future scope of Soviet research and development in this field, and, as a historical account, is revealing, and even startling at times.

The style is repetitive to an extreme, particularly when arguing the finer points of the position which the Soviets might have reached had they not abandoned most of their nuclear programme for two or three years to fight the Nazis. One senses that the author decided not to claim too much for them, yet in all honesty he had to admit the Soviets knew a thing or two—and earlier.

There is a bibliography, a well written index, and a set of notes on each chapter at the end of the work which could have been footnotes or at least presented with the relevant Chapter. I enjoyed reading this book from personal interest and association with the subject, but I would not consider it worth the price. British achievements are not played down, but they are rarely mentioned in the appropriate context until pages 150-151, when the civil power program receives " the oil."

C. R. TOTTLE.

## THE SCIENTIFIC PAPERS OF SIR GEOFFREY INGRAM TAYLOR, VOLUME II—METEOROLOGY, OCEANOGRAPHY AND TURBULENT FLOW

**Edited by G. K. Batchelor**

*London: Cambridge University Press. Pp. x + 515. Price 75s.*

This is the second of four volumes which overall will contain all the scientific papers of Sir Geoffrey Taylor that he and the editor, Dr. Batchelor, have been able to locate. The Cambridge University Press, the editor and the sponsors are to be congratulated again on the venture as a whole and on keeping the price to within relatively modest limits.

The first volume published some 18 months ago dealt with Sir Geoffrey's contributions to the mechanics of solids; the remaining three, of which the volume under review is the first, are devoted to fluid mechanics. The present volume is concerned with meteorology, oceanography and turbulence and contains 45 papers in all, set out in chronological order.

The appearance of the first volume was met with universal acclaim; the welcome for the second is no wit less cordial. It is a joy to see these volumes taking their place alongside those of Stokes and Rayleigh especially when fresh material continues to flow so copiously from the author's pen.

It is impossible to do more in the space of a short review than touch on a few aspects of the work set out; here are recorded so many of the ideas basic to our present thoughts and outlook .It is exciting to re-read a paper well known to one and to recall its impact at the time of its publication; it is just as exciting to read perhaps for the first time some gem, previously overlooked, more peripheral to one's immediate interests.

The first paper entitled " Eddy Motion in the Atmosphere " and dated 1915 contains the ideas basic to the vorticity transfer theory and it is amusing to find the author in 1932 in his paper on the transport of vorticity remarking, if not complaining, that he had originally put forward the theory in 1915 " though so briefly that it appears to have escaped notice."

Just as the transfer theories were basic to the developments on turbulence for the 20 years from 1915, so have Sir Geoffrey's ideas on the application of statistical principles to turbulence dominated the development since 1935. This work, foreshadowed by his classical paper on " Diffusion by Continuous Movements " in 1921, found its further expression in five remarkable papers entitled " Statistical Theory of Turbulence I-V." These papers and the extension of some of their ideas by von Karman and a later paper by Sir Geoffrey on isotropic turbulence revolutionized the whole study of turbulence.

Much of the work on meteorology and oceanography is, as might be expected, concerned with turbulent phenomena as for instance in the paper on Tidal Friction in the Irish Sea. This paper shows what can be done with a piece of paper, a pen, a map and information contained in routine Admiralty publications. The other main class of problem dealt with under these two headings is concerned with wave problems—atmospheric tides at the one extreme and at the other an excellent

fit to tidal data in the Bristol Channel derived from the standard theory for the propagation of waves in a channel of varying cross-section and the additional observation that the mean depth and breadth of the Bristol Channel vary linearly to a good approximation.

This in short is a book which all who devote their energies to fluid mechanics in any form and, indeed, anyone with a lively interest in physical science will wish to possess.

L. HOWARTH.

**LES SPECTRES ÉLECTRONIQUES EN CHIMIE THÉORIQUE**

**By C. Sandorfy**

*Paris : Revue D'Optique Théorique et Instrumentale. Pp. 230. Price 20.00 N.F.*

The literature on the theory of the electronic spectra of polyatomic molecules is very extensive. This little paper-backed book, therefore, meets a long-felt want in this field. The author assumes that the reader will be familiar with the symbolism and methods of wave mechanics. In the first section he gives a clear and concise summary of the two main theoretical methods used, the valence bond and molecular orbital approximations, and of the selection rules related to molecular symmetry which determine the intensities of the transitions.

The second and major section of the book is devoted to a survey of the results achieved by the theoretical methods in interpreting the observed spectra of a variety of molecules containing electrons in $\pi$ orbitals. The success of the " free electron model " calculations is reviewed and compared with LCAO treatments of the same molecules.

Professor Sandorfy concludes his monograph with a review of some of the wider correlations of ultra violet spectra with chemical properties including the effects of substituents, and the reactivities of ground and excited states.

This book deserves a place on the shelves of all research laboratories whose work encompasses the fields of quantum chemistry and electronic spectroscopy. It should prove a valuable source book for graduates commencing research in this subject. The references (260) cover the literature fairly thoroughly up to 1953.

C. G. CANNON.

# Forthcoming Institute Meetings

| *Date* | *Place* | *Speaker* | *Title* | *Time* | *Branch or Group* |
|---|---|---|---|---|---|
| May 18 | **Leicester**<br>Associated Electrical Industries Lamp and Lighting Co. Laboratories | | Visit to A.E.I. Lamp and Lighting Co. Laboratories followed by a lecture on **Artificial light sources for colour matching and allied problems** | | Midland |
| May 25 | **Newcastle-on-Tyne**<br>King's College | Professor W. E. Curtis, F.Inst.P., F.R.S. | Branch Annual General Meeting followed by a lecture on **The art of lecture demonstration** | 5.45 p.m. | North-Eastern |
| May 28 | **Reading**<br>The University | | Visit to the Physics Department of the University of Reading followed by a river trip to Henley. | | Education |
| | The number for this visit is limited and further details and application forms will be sent to members of the Education Group. | | | | |
| May 31 | **London**<br>The Institute's House | | All day meeting on **Experimental Stress Analysis** | 10.00 a.m. to 6.00 p.m. | Stress Analysis |
| | Registration is necessary. Full details may be obtained from the Secretary of the Institute. | | | | |

# Letters to the Editor

**The Internal Constitution of the Earth and Planets**

I have read with great interest Professor K. C. Bullen's article on the constitution of the earth, etc., published in the January, 1960, issue of the *Bulletin*.

There is a small error in the calculations on page 2, concerning the two spherical balls, *A* and *B*.

The moment of inertia of the ball *B* is stated to be about $\frac{8}{6}$ that of *A*. The actual fraction is $\frac{1}{2}\frac{0}{2}$, and $\frac{9}{7}$ would be a closer approximation than $\frac{8}{6}$. Hence $\frac{tB}{tA} = 0{\cdot}98$ (approx.)

Hence $tB$ is about 2% less than $tA$. The author quotes $3\frac{1}{2}$%.

R. C. ANDREWS (*Associate*).

20 High Park Avenue,
Wollaston, Stourbridge, Worcs.

**Graduateship Examination**

In recent months we have seen a spate of criticism directed against the Institute's Graduateship examination and examiners.

By way of contrast, may I be allowed to speak a word of praise and encouragement to the Institute and its examiners who are loyally, even courageously, striving to maintain the high professional and educational standards rightly demanded of those who seek corporate membership.

I would, however, call attention to the view held in some quarters that it is possible to secure exemption from the Institute's papers by virtue of examinations passed elsewhere at a lower standard. If this anomaly does exist, I trust that it will be removed by the new regulations.

Perhaps I should in fairness conclude by stating that I myself have never sat for any of the Institute's examinations, nor am I in any way connected with the preparation of candidates for this examination.

J. J. PARKER (*Associate*).

118 Coleshill Road,
Birmingham, 34.

# The Institute's Journals

## PRINCIPAL CONTENTS OF THE MAY, 1960, ISSUES

### *Journal of Scientific Instruments :* Volume 37, No. 5

**SPECIAL ARTICLE**

**The Physical Society's Exhibition—London, 1960.** By T. B. Rymer, F.Inst.P.

A general survey is given of the newer developments of instruments and apparatus exhibited, together with some observations on the Exhibition and the prize list of the Craftsmanship and Draughtsmanship Competition.

**Yarn-speed meter—a low-torque instrument for measuring the linear speed of low-inertia systems.** By D. L. Munden and T. K. Morley.

A small portable low-torque tachometer is described which has been designed to measure linear speeds of running yarns or threads. The instrument is a simple capacitor-switching circuit, power being provided by a small mercury-amalgam cell contained in the handle of the instrument. The instrument has been used to measure yarn speeds of up to 1200 ft/min to an accuracy of $\pm 1\%$ within the calibrated limits.

**Production and tensile testing of strain-free metal specimens.** By G. Greetham and A. J. Martin.

Details are given of equipment which has been used to prepare and mechanically test single crystal specimens of beryllium from zone refined rod. An electrolytic etching machine is described whereby the test-pieces may be prepared without mechanical deformation, and details are given of a modified Polanyi apparatus in which specimens may be tested in tension at temperatures up to about 600°C.

**Primary standard barometer of range 0 to 1200 mb.** By K. W. T. Elliott, D. C. Wilson, F. C. P. Mason and P. H. Bigg, F.Inst.P.

A primary standard barometer covering all pressures in the range 0 to 1200 mb has been designed and constructed at the National Physical Laboratory. A feature of the instrument is the use of optical probes as fiducial indicators which assess, from overhead, the positions of 11-cm diameter mercury surfaces in the barometer U-tube, thus permitting the mercury columns to be fully water-jacketed. The two probes use a single mercury pool as a common reference surface and their movements are assessed in terms of readings on associated line standards of Invar by means of fixed micrometer microscopes. The U-tube assembly is supported on an anti-vibration mount. The accuracy of height measurement of barometric columns was estimated by reference to measurements on end-gauges mounted in the empty U-tube and found to be better than 0·003 mm (standard deviation). Taking into account the small uncertainties in mercury density and local gravity the overall accuracy of a single pressure measurement is estimated to be about $\pm 0{\cdot}01$ mb on a 99% certainty basis. At about 1000 mb, check comparisons with N.P.L. primary barometers of earlier design support this assessment.

**Heat transport anemometer of high stability.** By A. J. Dyer, F.Inst.P.

An anemometer is described based on the principle of detecting the wake behind a heated wire by means of a thermometric bridge. An earlier design of this type of anemometer required a servo-amplifier having a gain of $10^6$ times. A new principle enables the desirable features of the former method, namely linearity and unidirectivity, to be preserved with an amplifier gain of 80 times with a corresponding improvement in stability and simplicity. The measured response time of the instrument is approximately 0·1 second.

**Vacuum sublimation apparatus for preparation of thin sources of α-active materials.** By N. Jackson.

This paper describes a method of preparing sources for α-pulse analysis, by subliming the oxide at 2000°C from a tantalum filament. Sources so prepared gave good uniform resolution with a peak width at half height of 35 keV or better.

**Vacuum furnace for high temperature X-ray diffractometry.** By J. N. Van Niekerk.

A vacuum furnace, to be used in conjunction with an X-ray diffractometer, for studying phase changes and solid state reactions at high temperatures is described. The sample is heated, primarily by radiation, from a platinum/20% rhodium heating element. The present temperature range is from room temperature to 1200°C, but a slight modification to the sample holder should allow temperatures of about 1600°C to be reached. Cooling of the furnace jacket is so efficient that simple O-ring principles are used to obtain the desired vacuum conditions. The sample holder is controlled from the outside and can be removed without in any way disturbing the rest of the furnace components. Data on temperature calibration and the experimentally determined inversion temperatures of quartz and iron are given. Decomposition studies of potassium and silver permanganates and phase changes in the iron carbide system, which are now in progress, illustrate the type of investigations which can be undertaken with this instrument.

**Apparatus for the zone refining of copper.** By E. D. Tolmie.

An apparatus has been constructed to enable a small molten zone to be formed and maintained at a constant length while being made to travel along a copper bar. Zone-refining performance is indicated for commercial O.F.H.C. copper after ten passes with speed of travel 11 mm/h.

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## *British Journal of Applied Physics :* Volume 11, No. 5

### SPECIAL ARTICLES

**Electron probe methods of X-ray microanalysis.** By P. Duncumb, A.Inst.P.

The use of an electron probe to produce a point source of X-rays permits chemical analysis, by X-ray absorption of fluorescence spectroscopy, of regions down to 1 to 10μ in diameter. For the study of surfaces, the emission method, in which the probe excites the specimen itself, is more suitable ; this gives a resolving power of about 1μ and a detection sensitivity of $10^{-14}$ g. By means of a scanning technique, an image of the surface can be formed with a selected emission line, so that the distribution of a particular element is presented visually. The three methods are described with typical applications and are shown to be in many respects complementary.

**Location of *p-n* and *l-h* junctions in semiconductors.** By P. A. Iles, A.Inst.P., and P. J. Coppen, A.Inst.P.

This article summarizes the properties of *p-n* and *l-h* junctions and the methods which have used these properties to locate them. Critical comments are made and criteria are given which must be considered when choosing a method, and some methods are recommended for the elements germanium and silicon. Lastly, an idea is given of the range of useful understanding of semiconductors which has resulted from use of these methods.

**Accuracy of selected-area microdiffraction in the electron microscope.** By A. W. Agar, F.Inst.P.

Because of the spherical aberration at the objective lens, the area of an object defined by the selector aperture for microdiffraction experiments does not exactly correspond to the actual parts of the specimen contributing to the pattern. Further errors may arise through incorrect lens settings. The magnitude of the possible errors and the electron-optical explanation are discussed. The theoretical results are in reasonable agreement with the experimentally observed errors. It is concluded that especial care is required in studies of thin metal foils where misorientations between grains are being measured.

**Vibration phenomena in detonation waves in hydrogen-oxygen mixtures.** By D. H. Edwards and T. G. Jones.

A photographic investigation is described, using a Schlieren system and rotating-drum camera, of detonation waves in hydrogen-oxygen mixtures propagating in a 1·6 cm diameter tube of circular section. It is found that mixtures near the limits of detonability exhibit the phenomenon of spin in which the motion is of long wavelength and the frequency is in agreement with values predicted by existing theories. In stronger detonating mixtures a high-frequency spin of short wavelength is present which shows the same characteristics on a streak photograph as a normal long wavelength spin ; measured values of frequency and wavelength agree reasonably well with those calculated. In a range of mixtures of intermediate composition both types of vibration are present. Longitudinal pressure waves, the recurrence frequency of which is twice that of the fundamental mode transverse vibration, are also observed behind the detonation front in all mixtures ; the existence of these waves is confirmed by pressure recordings and a possible explanation of their origin is discussed.

**High alloy steels for use as a thermal conductivity standard.** By R. W. Powell, F.Inst.P., and R. P. Tye.

An absolute longitudinal steady state heat flow method is described for the determination of the thermal conductivity of a bar of Macloy G steel over the range 0 to 180°C. The results of this method are in good agreement with those of a comparative method against Armco iron that is used for the range 300 to 800°C and values are quoted for the full range which are believed to be accurate to about ±2%. Comparison is made with some previous determinations on a sample of Era ATV steel.

These steels are suggested as suitable for use as standard materials for comparative thermal conductivity determinations on metals of low conductivity.

**Correction of errors in electron stereomicroscopy.** By O. C. Wells.

A method is described for analysing electron stereomicrographs. *Three* position co-ordinates are computed for each object point from the *four* co-ordinate measurements (two from each micrograph) that are available for each point from the stereo pair. The method allows for the correction of tilt error and perspective error, and for the estimation of the magnitude of the errors that remain. An example of the application of the method is described.

**Contact electrification and polarization of nylon threads.** By R. G. C. Arridge, A.Inst.P.

A study has been made at different relative humidities of the way in which electric charges on nylon threads decay. For relative humidities of less than about 35%, the charges remain localized and decay in magnitude at very nearly an exponential rate. For relative humidities greater than 35% the charges spread out along the thread and the decay of peak charge with time can be approximately represented by a $t^{-1/2}$ law rather than by an exponential one.

If a diffusion equation of the type $p = At^{-1/2} \exp(-x^2/4\,Dt)$ is applied to the latter results, values of the diffusion constant $D$ are obtained which are independent, within the accuracy of the experiment, of (i) the sign of charge and (ii) whether it was a contact charge or a polarization charge. $D$ varies exponentially with relative humidity.

**Resistance-temperature relationship of platinum at low temperatures and its influence on precision thermometry.** By G. C. Lowenthal, A.Inst.P., and A. F. A. Harper, F.Inst.P.

An analysis of the results of measurements of the resistance of platinum as a function of its temperature and purity indicates that known weaknesses in the Callender-Van Dusen equation can be explained by the presence of a point of inflexion in the resistance-temperature curve near 86°K, only a few degrees below the present lower limit of the International Temperature Scale (I.T.S.). A satisfactory interpolation formula for temperature measurement below 90°K can be obtained if account is taken of the fact that $dR/dT$ for platinum is the greater the purer the metal only above about 26°K, while the reverse is true below that temperature. This suggests the use of the normal boiling point of neon (27·07°K) as an intermediate fixed point. If this point should prove adequately reproducible, it should prove practicable to extend the I.T.S. by means of the simple so-called Cragoe function from 90 to 27°K, preferably with a break in the range at the triple point of oxygen (54·36°K).

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**Applications :** Forms obtainable from the Senior Representative, (AV.58/3), Department of Supply, Australia House, Strand, London, W.C.2, with whom completed applications should be lodged by May 31, 1960.

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B.I.P. Chemicals Ltd. have vacancies for **Scientists** and **Technologists** in their laboratories at Oldbury. The appointments will be in the Research Development, or Analytical and Testing Departments, which are undergoing considerable expansion, and opportunities will arise later for transfer to other Departments, or to other Companies within the British Industrial Plastics Ltd. Group. The desirable minimum qualification is a good honours degree, A.R.I.C. or A.P.I., and the preferred age range is 21-35. Post-graduate or industrial experience will be an advantage for some of the appointments.
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Applications are invited for the following appointments : Senior Lectureship in Physics, Lectureship in Physics, 2 Lectureships in Mathematics. The salary scales are : Professors £2,800, Senior Lecturers, £1,750 – £2,000, Lecturers £1,250 – £1,700. An allowance is made towards travelling expenses. Further particulars and information as to the method of application must be obtained from the Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1. Applications close, in New Zealand and London, on June 15th, 1960.

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BP requires PETROLEUM ENGINEERS for overseas service in its Exploration Department. Applicants, aged between 20-27, must possess an honours degree in physics, mechanical engineering or petroleum engineering. Previous oilfield experience would be an advantage. Salary according to age, qualifications and experience. National Service obligations must have been fulfilled. Write, stating age and full details of qualifications and experience, quoting F.579, to The Manager, Recruitment Branch, The British Petroleum Company Ltd., BP House, Ropemaker Street, London, E.C.2.

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AUSTRALIA

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NEW ZEALAND

DEPARTMENT OF PHYSICS

Applications are invited for the following positions :—LECTURER : Candidates should be qualified and interested in any of the following research topics : solid state studies, X-ray spectrography of solids and gases, radio investigations of the upper atmosphere such as meteorite distribution, auroral zone detection and studies of the lower ionosphere. ASSISTANT LECTURER : Candidates should have an honours degree in physics. The salary for a Lecturer is £1,250 × £75 – £1,700 per annum and for an Assistant Lecturer £900 × £50 – £1,100 per annum. Commencing salary in each case will be in accordance with qualifications and experience. Approved fares to Christchurch will be allowed for an appointee, his wife and children, together with actual removal expenses within certain limits. Further particulars and information as to the method of application may be obtained from the Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1. Applications close in New Zealand and London on June 30, 1960.

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